

PROCEEDINGS
OF THE
ROYAL SOCIETY OF EDINBURGH.

VOL. II.

1850.

No. 39.

Monday, 4th March 1850 (continued).

The following Gentlemen were duly elected Ordinary Fellows:—

Lieut. W. DRISCOLL GOSSET, Royal Engineers.

Dr WILLIAM SELLAR, Pres. R.C.P.E.

The following Donations to the Library were announced at the Meeting of 18th February:—

The London University Calendar. 1850. 12mo.—*By the Publishers.*

The American Journal of Science and Arts. Conducted by Professors Silliman and Dana. Vol. IX., No. 25. 8vo.—*By the Editors.*

Mémoires de l'Académie Impériale des Sciences de St Pétersbourg.
Sixième Série. Sciences Mathématiques, Physiques et Naturelles. Tome VIII^{me}, 2^{me} partie. Sciences Naturelles.
Livraisons 3^{me}, 5^{me}, et 6^{me}. 4to.

Mémoires présentés à l'Académie Impériale des Sciences de St Pétersbourg, par divers Savants et lus dans ses Assemblées.
Tome VI^{me}. Livraisons 2^{de} et 3^{me}. 4to.—*By the Academy.*

Messungen zur Bestimmung des Höhenunterschiedes zwischen dem Schwarzen und Caspischen Meere, von G. Fuss, Sawitsch und Sabler. 4to.—*By the Authors.*

Rapport fait à l'Académie Impériale des Sciences de St Pétersbourg,

VOL. II.

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par W. Struve. Sur une Mission Scientifique dont il fut chargé en 1847. 4to.—*By the Author.*

W. Struve sur la Delatation de la Glace d'après les expériences faites en 1845 et 1846 à l'Observatoire Central de Poulkova, par MM. Schumacher, Pohrt, et Moritz. 4to.—*By the Authors.*

Über Prof. Mädlers Untersuchungen über die Eigenen Beweyungen der Fixsterne, von C. A. F. Peters, Dr. 4to.—*By the Author.*

P. H. Fuss Nachricht über eine Sammlung Unedirter Handschriften Leonhard Eulers, und über die Begonnene gesammtausgabe seiner Ueineren schriften. 8vo.—*By the Author.*

Über die Genanig-keit der in Lalandes Catalog, publicirt von der *British Association*, enthaltenen Sternörter, von Dr Lindhagen. 8vo.—*By the Author.*

Verhandlungen der Schweizerischen Naturforschenden Gesellschaft bei ihrer Versammlung zu Slothurn. 1848. 8vo.—*By the Society.*

Mittheilungen der Naturforschenden Gesellschaft in Bern, aus dem Jahre, 1848–9. Nos. 135–161. 8vo.—*By the Society.*

The following Donations to the Library were announced at the Meeting of 4th March :—

Transactions of the Cambridge Philosophical Society. Vol. VIII. 4to.—*By the Society.*

The Astronom. Jour. Vol. I., Nos. 2, 3, & 4. 4to.—*By the Editor.*

Proceedings of the R. Ast. Soc. Vol. X., No. 3. 8vo.—*By the Society.*

Proceedings of the Linnæan Society of London. Nos. 30–40. 8vo.

Charter and Bye-Laws of the Linnæan Society. 1848. 8vo.

List of the Linnæan Society. 1849. 4to.—*By the Society.*

Journal of Agriculture and Transactions of the Highland and Agricultural Soc. of Scotland. No. 28, N. S. 8vo.—*By the Society.*

Annales des Sciences Physiques et Naturelles, d'Agriculture et d'Industrie, publiées par la Société Nationale d'Agriculture, &c., de Lyon. Tom. II. 1848. 8vo.—*By the Society.*

A Collection of Maritime Charts, with corresponding Descriptions. —*By the French Government.*

Monday, 18th March 1850.

The Right Rev. BISHOP TERROT, V.P., in the Chair.

The following Communications were read :—

1. Note regarding the American Electric-Observing Clocks.
By Professor Piazzi Smyth.

The object of this communication was chiefly to exhibit a specimen of the register of the electric chronograph, wherein the second's beats of two clocks were marked side by side, one going nearly to sidereal time, and the other to solar; and the length of a second's interval on the paper was so great, and the accuracy of the punctuation such, that the minute acceleration of the one clock on the other could be registered almost from second to second.

The electric register can be applied with ease to any clock, and at any distance from the recording apparatus; and two or more clocks, or they may be simple pendulums, can be made to register their vibrations on the same slip of paper.

The author pointed out how this method might be made available for determining the density of the earth, by observations on the shores of the Bay of Fundy, during the rise and fall of the enormous tides which occur there. He likewise mentioned several purposes to which Lieutenant Maury, U. S. N., proposed to apply the electric chronograph; amongst others, to determining the height of mountains, as he thought that the accuracy capable of being attained in determining the time of vibration of a pendulum in this manner, was so extreme, that the method might be safely applied to such problems.

2. Account of a Remarkable Meteor, seen 19th December, 1849. By Professor J. D. Forbes.

" On the evening of the 19th December 1849, whilst walking near the southern part of Edinburgh, about fifteen minutes past five. Greenwich time (as I afterwards estimated), I observed a meteor, fully brighter than Venus at her average brilliancy, moving from W. towards N., parallel to the horizon, elevated 15° above it, and followed by a distinct luminous train. This angle was subsequently

taken by estimation by daylight, with the aid of a theodolite; and the compass-bearing of the meteor, when first seen, ascertained in the same way, must have been 47° W. of N. When it bore 29° E. of magnetic north, it was observed to have divided into two, the one part following the other at some distance; and I soon after lost sight of it in the obscurity of the smoke of the town. When it split, its altitude was estimated at 6° . It thus described an arc of no less than 76° , in doing which it occupied, as I roughly estimated, about 15 seconds, or possibly more.

“ Having sent a short notice of the appearance of the meteor to the Courant newspaper, I received from many quarters accounts of its having been seen under circumstances remarkably similar to those just described. I believe that nearly forty communications on the subject have reached me from places included between Longford, in the centre of Ireland, to near Bervie, in Kincardineshire, a distance of above 300 miles, in a direction nearly NE. and SW., whilst in a perpendicular direction, or from NW. to SE, the range of observation has been comparatively small; for I have received no information from beyond Renfrew, in the one direction, and Durham in the other; being about 140 miles distant in a straight line. The meteor was seen at Longford, in Ireland, 74 miles west of Dublin, but not in Dublin itself. It was seen at Belfast, between Carlisle and Gretna at Stewarton in Ayrshire, at Johnstone, at Paisley, Renfrew, and by many persons in Glasgow and the neighbourhood. It was also generally seen in Edinburgh, in East Lothian, near Melrose, and at Durham, as already mentioned. Further north, I have received accounts from Crail, St Andrews, Dundee, Perth, and Johnshaven to the north of Montrose.

“ The greater number of these communications concur in estimating the direction of the motion of the meteor to have been from SW. to NE., although, as might be expected, they vary excessively as to its distance and magnitude; being described by some persons as only 50 or 100 yards off, and as large as the moon; by others, as a ball of 9 inches in diameter, or the size of a large egg. One person only professes to have heard a sound. The time during which it was seen was variously estimated. At Longford, by Mr Curtis, 20 sec.; at Glasgow, by Mr Stevenson, at 20 sec.; at Johnstone, by Mr Cunningham, 15 sec.; at Perth, 15 or 20 sec.; at Durham, by Mr Carrington, 30 sec.; at St Andrews, 15 seconds according to one

observer, and 18 to 21 seconds according to another ; at Johnshaven, $\frac{3}{4}$ ths of a minute. The hour of the appearance of the meteor, in most of the descriptions, is stated at between 5h. 10m., and 5h. 16m.

“ The arc of the horizon which it was seen to traverse depended, of course, on the point where the meteor first caught the observer’s eye. At Granton, it was traced by Professor Kelland through 125° of azimuth ; at Perth, 130° ; at St Andrew’s, 74° ; at Edinburgh, 76° ; at Durham, 65° ; at Glasgow, from 60° to 70° . The division of the head or nucleus into several parts, and, first of all (in most cases), into *two*, has been noticed with remarkably slight variation ; consequently, the explosion of the meteor marks a well-determined point in its path. The separation was specially noticed at Edinburgh, Granton, Glasgow, Renfrew, Melrose, Haddington, Johnshaven, Perth, Durham, St Andrews.

“ In a majority of cases a luminous train was observed ; and I am confident, that the existence of this train, which has been estimated at from 2° to 3° long, cannot be questioned. Dr Adamson, however, especially remarked that no train was to be seen at St Andrews.

“ On revising the whole accounts, it does not appear that any of them can be relied upon, for ascertaining the position of the meteor in space, except the observations of Mr Carrington of the Durham observatory ; of Professor Kelland, Mr Stirling, and myself, at Edinburgh ; of Dr Adamson and another observer, communicated by Professor Fischer of St Andrew’s ; of a young gentleman at Perth, communicated by Thomas Miller, Esq., Rector of the Perth Academy ; and of A. D. Stevenson, Esq., and W. Gourlie, Esq., junior, at Glasgow. My inquiries were chiefly directed to the two following points : *first*, the angular elevation of the meteor in the NW. quarter of the heavens, where it is admitted by all that its path appeared almost horizontal ; *secondly*, to the bearing of the meteor at the instant of explosion.

“ At Durham, Mr Carrington saw the meteor first when the bearing was true NW., the altitude (by theodolite) was then 10° , or not exceeding 11° ; when it burst, it was due N. (true), and continued to move 10° or 12° further before it disappeared. Professor Chevallier, who obligingly communicated these results, states that the meteor appeared rather to rise as it approached the north, but with a doubt. This supposition, however, appears inadmissible, from the unanimity of the other accounts.

"At Granton, near Edinburgh, Professor Kelland caught sight of the meteor a little to the N. of the moon, and several diameters below it. This corresponds, by after estimation with a theodolite, to 75° W. of magnetic N., and an altitude of 12° . Professor Kelland thinks that it rather rose afterwards. It split into two at 20° E. of magnetic N., having then an altitude of only 5° ; it continued for a considerable time bright, then began to fade, as if by the effect of distance, and also to separate into several parts: it was finally lost sight of 50° E. of magnetic N. (this bearing is well ascertained), with an altitude estimated at only half a degree. The position and circumstances of these observations, made at an elevated station above the Frith of Forth, were eminently favourable.

"Mr J. Stirling, civil engineer, looking up North Hanover Street, Edinburgh, saw the meteor separate into two parts; the bearing he afterwards estimated at 25° E. of magnetic N. (the probable error not exceeding 1°), and the altitude at $8^{\circ} 30'$, certainly not exceeding 9° .

"I think we may conclude, that at Edinburgh the meteor attained a maximum elevation of 15° (that mentioned in the commencement of this paper), since it no doubt rose after Professor Kelland first saw it to the S. of the true W., with an altitude of only 12° . The course of the meteor was evidently such as to be nearest the spectator when in the true NW. or WNW.

"The place of the meteor when it burst stands thus:—

Kelland, N. 20° E. (mag.)	Alt. 5° .
Stirling, N. 25° E.	Alt. $8^{\circ} 30'$.
Forbes, N. 29° E.	Alt. 6° .

"The average is almost 25° E. of N., or about 1° W. of the true meridian, the variation being nearly 26° . The mean of the three observations of altitude would be $6^{\circ} 30'$; but admitting Mr Stirling's to be entitled to the greatest confidence, we may suppose it 7° , or possibly a little more.

"At St Andrews, the meteor was seen by Dr Adamson, when riding in a northerly direction, on the Largo road. Professor Fischer was so kind as to accompany him afterwards to the spot, and to reduce his observations with all the accuracy of which they were capable. It was first noticed when bearing $8\frac{1}{2}^{\circ}$ W. of magnetic N., and disappeared at $42\frac{1}{2}^{\circ}$ E. of N.; the altitude was conjecturally

stated as between 14° and $18\frac{1}{2}^{\circ}$, and it appeared to move horizontally, but rather declining towards the N.

“ After describing three-fourths of its course, it split into two parts, which went on close together for a little, then broke into four or five, became dull red, and rapidly disappeared ; the separate pieces travelling on together until the last.

“ Another intelligent observer near St Andrews, whose evidence was taken by Mr Fischer, first saw the meteor $29\frac{1}{2}^{\circ}$ W. of magnetic N., and estimated the point where the meteor burst at 44° E. of N. ; but this last number coincides so closely with Dr Adamson's estimate of the point of final disappearance, that it is perhaps allowable to suppose, that this second observer had mixed up these two events in his description. Dr Adamson's statement, that one-fourth of the arc which he saw was described after the meteor had split, would give an azimuth at that moment of almost 30° E. of N. magnetic, or 4° E. of N. true, as Mr Fischer determined the magnetic declination to be about $25^{\circ} 46'$. The altitude of the meteor, as seen by this observer, appears not to have exceeded 15° (the same as at Edinburgh) ; which number we shall therefore adopt.

“ At Perth, the passage of the meteor was seen from the North Inch, by a young gentleman of intelligence, whose observations were reduced to numbers by Mr Miller, Rector of the Perth Academy, who was so good as to accompany him to the spot, and take the angles with a theodolite. Its bearing, when first seen, was 46° S. of W. true ; its angular altitude was at that time only $3^{\circ} 30'$. This is by far the most southern azimuth which has been observed. Its bearing, when it disappeared, was 6° W. of N., but it was then lost in a cloud. If I understand right, it had, by this time, separated into fragments. Its apparent altitude, in the middle of its course, was about $17^{\circ} 30'$. These observations, extending over an arc of 130° , taken along with Professor Kelland's, clearly demonstrate that the meteor appeared with a very low altitude in the SW. quarter of the heavens, and disappeared in a similar way in the NNE., attaining its greatest elevation about WNW. (true.)

“ At Glasgow the meteor was very generally and well seen. Mr William Gourlie junior saw it move from SW. to NNE., over an arc of 60° or 70° , and divide into two, when it bore 40° E. of magnetic N. He estimates its greatest elevation at 30° , and that it

decreased to between 15° and 17° , or even less, at the time of its separation. He adds, that he is not much accustomed to such observations. Mr A. D. Stevenson, living in South Portland Street, Glasgow, saw the meteor moving along, at a height just sufficient to clear the chimney-tops, on the west side of the street, an elevation which he afterwards estimated, as he states, with considerable accuracy at 28° . I have received farther and more minute accounts of the appearance of the meteor from Mr Stevenson, who has been most kind and intelligent in his communications; and my friend Mr James Peddie has verified the accuracy of Mr Stevenson's observations beyond the possibility of mistake. It appears that the meteor passed quite clear of a stack of chimneys on the opposite side of the street, which would give it a well-defined minimum altitude of $25^{\circ} 41'$; but Mr Stevenson is of opinion that it rose more than 2° higher, or to not less than 28° (perhaps even to $28^{\circ} 21'$); when it was highest, its bearing was $52\frac{1}{2}^{\circ}$ W. of N. (magnetic), and it disappeared from his view when it bore $40^{\circ} 27'$ E. of magnetic N. *It was then decidedly single.* Now, this bearing coincides with that at which Mr Gourlie observed it to become *double*; and, consequently, the limit towards the N. of this event is severely defined.

“ The following Table contains the most definite of these observations, and the azimuths are all reduced to the true meridian :—

	Greatest Altitude.	True Azimuth when first seen.	True Azimuth of disap- pearance.	Arc ob- served.	True Azimuth of first explosion.	Altitude at first ex- plosion.
Durham,	$10^{\circ} 30'$	N. 45° W.	N. 12° E.	57°	N.	
Edinburgh,	15°	W. 11° S.	N. 24° E.	125°	N. 1° W.	7°
St Andrews,	15°	N. 55° W.	N. 16° E.	71°	N. 4° E.	
Perth, .	$17^{\circ} 30'$	W. 47° S.	N. 7° W. (in a cloud)	130°	?	
Glasgow,	28°			$100^{\circ}?$	N. 14° E.	15°

Remarks on the Observations.

" 1. On the whole, these observations are not consistent, and cannot (I conceive) be cleared up without additional and accurate ones, which it may now be too late to procure. The central group of stations, Edinburgh, Perth, and St Andrews, are sufficiently accordant, and indicate that the path of the meteor must have been nearly parallel to a line passing through the first and last of those places, or in a direction N. 27° E. (true); which accords well with the observations at most of the individual stations, and particularly with the *vanishing direction* in Professor Kelland's remarkable observation at Granton.

" 2. The Durham observation is compatible with the above-mentioned group within the limits of error. By the combination of Durham and Edinburgh (the base line perpendicular to the assumed direction of the meteor's motion being 95 miles), I calculated that the meteor passed vertically nearly over the Island of St Kilda, with an absolute elevation of about 88 miles. But this solution seems absolutely excluded by observations at Glasgow which admit of no question, and which I have spared no pains in verifying. Had the position of the meteor been such as I have first assumed, it could not possibly have been seen over even the roofs of the houses from the station occupied by Mr Stevenson, much less over the chimneys-tops. The bearing, at the moment of explosion at Glasgow, also singularly enough corroborates sufficiently well the comparatively small elevation (about 20 miles above the earth) which the combination of Edinburgh and Glasgow gives; and this bearing we have seen to have been also accurately defined by the physical obstacles bounding the observer's view; it would have given a parallax of 15° , subtended by the perpendicular on the meteor's path, referred to Glasgow and Edinburgh respectively. Now, if this calculation were anything like correct, the Perth observation is entirely wrong; and the meteor could not have risen about 6° above the horizon of Durham, instead of 10° or 11° as estimated. I am unable, in any degree, to explain these conflicting results.

" 3. The observations of Professor Kelland at Granton, and those at Perth, through the great azimuths of 125° and 130° , described by the meteor with such remarkable deliberation of motion, lead, when analyzed, to the very same results which presented themselves to the

mind of the spectator intuitively ; namely, that the motion must have been sensibly rectilinear, equable, and parallel to the horizon at Edinburgh. Assuming that the greatest altitude at Edinburgh was 15° , and the bearing then N. 63° W. (true), we may calculate that the altitude should have been on this hypothesis, when first seen by Professor Kelland, $11^{\circ} 47'$,—instead of 12° as observed ; at explosion, $6^{\circ} 59'$ (7° observed), and at its final disappearance $0^{\circ} 47'$ (instead of $0^{\circ} 30'$ observed). Again, at Perth the observed altitude, when first seen, was $3\frac{1}{2}^{\circ}$, and the calculated altitude $5^{\circ} 3'$, taking the maximum altitude at $17\frac{1}{2}^{\circ}$. The coincidence is, on the whole, remarkable, though it would be rash to push it to an extreme, as an error of some degrees may exist in the assumption of the direction of the meteor's course. Some later observations, received from Mr Curtis at Longford, and a consideration of the effects of perspective at Perth and Edinburgh, incline me to admit that the path might make an angle 3° or 4° greater with the meridian than I have above supposed. These conclusions are independent of the actual distance or parallax of the meteor ; which, as I have said, cannot be determined without further observations, which I should be glad to receive from any quarter, but more particularly from Ireland, and from the centre and NW. of Scotland. If correct, they entitle us to infer that the meteor in question was most probably a body moving in space, in a path little curved, and not revolving round the earth."

3. Notes on the Purification and Properties of Chloroform.

By William Gregory, M.D., Professor of Chemistry in the University.*

1. Chloroform has been prepared both from alcohol and from wood-spirit. The latter has been used for the sake of cheapness ; but as it is a mixture of several liquids, all of which do not yield chloroform, it gives an impure product, in a proportion which varies much, but is always below that obtained from alcohol. There is

* Although I am alone responsible for the opinions contained in this paper, it is my duty to state, that all the experiments and observations mentioned in it have been made by me in concert with my able assistant, Mr Alexander Kemp, of whose ingenuity and accuracy I have had constant opportunities of judging.

therefore not only no advantage, but the contrary, in using wood-spirit, which is not, after all, much cheaper than alcohol.

2. But the chloroform from these two liquids, *when fully purified*, is quite identical in all its properties. Its smell, density, boiling point, and action on the system are, in both cases, exactly the same. That from alcohol is, no doubt, more easily purified than the other; but it also contains volatile oily impurities, which must be removed before it can be safely used. The peculiar oils which adhere to both kinds of chloroform are not identical, or, at least, not all identical; but they are of analagous constitution and properties.

3. Soubeiran and Mialhe have examined these oils. They contain chlorine, have a disagreeable smell, and, when inspired or smelt, cause distressing headache and sickness. In the case of wood-spirit, some of its own impurities distil over unchanged, and are found in the chloroform.

4. It is well known that many persons, after the use of chloroform, have suffered from headache, nausea, and even vomiting, as I have more than once seen. Headache and nausea I have myself experienced, when I have tried different specimens of chloroform, without taking so much as to produce the full effect.

5. Perfectly pure chloroform, such as is now on the table, does not, so far as I have seen or experienced, produce these disagreeable effects. It is, therefore, highly probable that when they occur, as they do with some individuals, from the use of chloroform of more than the average goodness of quality, this depends on the presence of a trace of these poisonous oils.

6. All good manufacturers of chloroform purify it by the action of oil of vitriol; which destroys the oils, while, at the same time, a part of the acid is reduced to sulphurous acid. The chloroform, to remove this, is then distilled with lime or carbonate of baryta, and is tolerably pure, if the process be well conducted.

7. But this is not quite pure, and contains a trace, more or less distinct, of the oils. I have found this to be the case with all the best chloroform made here, up to 1849; and I have several times seen headache and sickness from the use of such chloroform, which, as we all know, was the best anywhere made. I must add, however, that the quantity of oils was, although variable within certain limits, always, in the Edinburgh-made chloroform, so small, that it was fit for use, and only caused headache, &c., in a few peculiarly sensitive persons.

8. It was desirable to have a test for these impurities, as well as an easy and effectual mode of removing the last traces of them; especially as many sorts of chloroform, not made here, were far inferior in quality to that prepared in Edinburgh. One very delicate test is, that oil of vitriol, which should be quite colourless and pure (as it may be rendered by Mr Kemp's process, lately read to the Society), when agitated with the chloroform, becomes yellow or brown, from its action on the oils, which it chars and destroys. Any change of colour is easily seen by the contrast with the colourless chloroform which floats above. Pure chloroform gives no colour to the acid. It is essential that the oil of vitriol be colourless, and also of full density; for, if coloured, it is not easy to see a slight change in its colour; and if below the proper density, that is, too weak, it is not much coloured by a chloroform which will render brown the acid of proper strength.

9. Another test, still more delicate, I find to be the smell of the oils. When chloroform is poured on the hand or a handkerchief it rapidly evaporates; but the oils, being less volatile, are left behind, and their smell, previously covered by that of the chloroform, is easily recognised. Until very lately, no chloroform was sold, or, indeed, known, which would stand this test, or even the former.

10. Up to 1849, the best commercial chloroform had a specific gravity of 1.480, which was considered a guarantee of its purity. But it had been obtained, by chemists, of specific gravity 1.494 and even 1.497. I have found that chloroform of 1.480, when once more acted on by oil of vitriol, which destroys the oils and becomes brown, may be obtained, after removing the sulphurous acid, of specific gravity 1.500 at 60°. This I take to be the specific gravity of pure chloroform. Our best makers have lately, much to their credit, pushed the purification so far as to furnish chloroform even of this highest density, and also, in other respects, such as it ought to be.

11. There are still, however, many makers, in other places, whose chloroform is not so pure; and I shall now describe the method which, with Mr Kemp, I have employed for purifying, perfectly and easily, any commercial chloroform (except one remarkable specimen, of which more hereafter), a process which will enable any medical man to purify it for himself with the greatest facility.

12. The chloroform, having been tested as above, and found more

or less impure, is to be *agitated with* oil of vitriol (half its own volume will be sufficient), and *allowed to remain in contact with the acid*; of course in a clean, dry, stoppered bottle, and *with occasional agitation*, till the acid no longer becomes darker in colour. As long as the action is incomplete there will be seen, after rest at the line of contact, a darker ring. When this no longer appears, the chloroform may be drawn off, and, for greater security, once more acted on by a quarter of its volume of the acid, which should now remain colourless. It is now to be once more drawn off, and, in a dry stoppered bottle, mixed with a little powdered peroxide of manganese, with which it is gently agitated and left in contact, until the odour of sulphurous acid is entirely destroyed, and the chloroform has acquired a mild agreeable fruity smell. It has then only to be poured off into a proper phial. It will now leave no disagreeable smell when evaporated on the hand. (If the commercial chloroform, after having been *frequently well shaken*, and *left for some time in contact* with the acid, has given only a moderate tinge of colour to it, it is probable that it may be completely purified by that first process. To ascertain this, test a small portion in a tube with fresh acid, *shaking well*, and *allowing it to stand some time*. If it do not colour the acid at all, then the whole chloroform has only to be finally purified by the oxide of manganese. If the acid become coloured in the test tube, it will be as well to act on the whole chloroform a second time with fresh acid, till it stands the test. Mr Kemp has observed, in repeating this process for me, the very curious fact that, as soon as the action is complete and the oily impurities are destroyed, but not sooner, the chloroform tested with the acid in a tube exhibits a strongly convex surface downwards, where it rests on the pure acid, or, what is the same thing, the acid becomes concave at its upper surface. The smallest trace of impurity, not sufficient to affect the density of the chloroform, we have found to render the line of junction horizontal. It is probable that this may become a valuable test of the perfect purity of chloroform, but we shall not say more on this subject until we have thoroughly examined it.)

This process requires no apparatus beyond a few stoppered bottles, and a syphon, or a pipette, if we wish to draw off the whole chloroform without loss. The use of the oxide of manganese is due to Mr Kemp; and, on the large scale, the chloroform may be filtered

through a cylinder full of it. In this final purification of genuine, although not quite pure chloroform, no distillation is necessary.

13. It may be considered as certain, that the use of chloroform, thus purified, will very rarely, if ever, cause the disagreeable effects above noticed.* As to more serious bad results from the use of chloroform, so often spoken of elsewhere, it is enough to state, that a large proportion of the cases must be attributed to the use of a liquid so impure, as hardly to deserve the name of chloroform at all. Such a product, I rejoice to say, our Edinburgh manufacturers have never sold; and, I may add, that, no doubt chiefly in consequence of this, our practitioners have not yet seen a fatal result from the use of chloroform. But in London, and elsewhere, chloroform has been extensively sold, so bad, that I have examined specimens which did not contain half of their bulk of chloroform; others with not one third or one fourth; and I have seen one which hardly contained any at all. But, to make up for this, they were rich in poisonous oils, and

* Dr Simpson informs me, that the purest chloroform he has used not unfrequently causes vomiting. On further inquiry I find that this occurs when it is administered after a full meal. This can easily be avoided, and must not be confounded with the headaches, nausea, and vomiting alluded to in §§ 4 and 5; which symptoms are persistent, and occurred, in my experiments, always with an empty stomach, the experiments being made an hour or two before dinner. Dr Carmichael, assistant to Dr Simpson, has mentioned to me some facts which confirm the view I have taken. At one period, for more than a week, Dr Simpson and Dr Carmichael were kept in a state of continual anxiety by the occurrence, in all the puerperal cases in which chloroform was used, of very unpleasant symptoms, particularly of frequent pulse and other febrile symptoms, lasting for some days. At last, after much annoyance from this cause, it occurred to Dr Simpson that he was using one particular specimen of chloroform, supposed to be of good quality. As soon as this idea occurred, he threw away all that remained, and returned to that which he had generally used. The unpleasant symptoms no longer appeared. (I regret much that I had not an opportunity of examining that specimen; but I may add that the maker, not an Edinburgh one, now produces chloroform of much better quality, though not yet absolutely pure.) But the striking fact is this, that Dr Simpson and Dr Carmichael state, *that during the period above alluded to, when that one kind of chloroform alone was used by them, their handkerchiefs became quite offensive from the smell left on them, which even adhered to them after washing.* There can, I think, be no doubt that here the oily impurities alluded to in §§ 4 and 5 were present in notable quantity. I suspect that a majority of the specimens mentioned in the Table would have a similar effect, more or less marked. (I have since ascertained that this chloroform, which was much above the average in quality, had not been subjected to the action of oil of vitriol in its preparation, which strongly confirms the view I have taken. W. G.)

often in free hydrochloric acid. Very many specimens, although better than this, are yet so impure, that no one could, with comfort or safety, use them.

14. The chloroform now, and for some time past, made here, is of first-rate quality. I have two specimens which are absolutely pure, or nearly so; and a third, which is hardly inferior, all made and sold by Edinburgh manufacturers.

15. On the other hand, I have various specimens, maker unknown, besides some from makers in other places, which are not so pure, although, in general, much purer than those which I examined nearly three years ago. But one specimen deserves a separate notice. It is labelled "*pure chloroform.*" It is yellowish, has a strong smell of the oils, and of impure wood-spirit; and, when treated with its own volume of oil of vitriol, develops much heat, colours the acid dark brown, and disappears almost entirely, any trace of chloroform it may contain being boiled off by the heat disengaged. It contains also so much free acid, that the cork is corroded. It is to be hoped that this product disgraces no longer the market. I do not know the name of its maker. Three of the specimens became milky, when mixed with the acid. One, after contact with the acid, acquired a strong smell of musk. Another lost about a third of its bulk. All but two coloured the acid decidedly at once; and all left, more or less, a disagreeable smell on the hand. One of the two which did not much colour the acid at first was that which acquired the smell of musk; the other, evaporated on the hand, left a white stain, depending partly on the matters present in the skin. This was the case also with another; yet these two coloured the acid but little at first, more strongly after a time: but both left a smell on the hand. Only one (Edinburgh made) specimen, of density 1.500, gave no colour, or only a perceptible tinge, to the acid.

16. In conclusion, I would remark, that while the use of chloroform in Edinburgh, in many thousand cases, has never yet led to a fatal result, I do not intend to maintain that the use of pure chloroform never can cause fatal effects. On the contrary, I have no doubt that, if rashly, carelessly, or ignorantly administered, so powerful an agent may, like any other powerful drug, especially in individuals of peculiar temperament, and in cases of severe, though latent internal disease, give rise to fatal results. That no such cases have here been met with is due partly to the good quality of the

chloroform used, and to the care with which it is prepared; and partly to the experience and judicious management of those whose duty it is to administer it, at the head of whom stands the introducer of chloroform, my friend and colleague, Dr Simpson.

It is much to be regretted that, in London and elsewhere, chloroform is not by any means so extensively employed as it ought to be, in consequence of the occurrence of some fatal cases, attributed (whether in all cases accurately or not, is a question) to the drug. There can be no doubt that most, if not all, of these cases have resulted from the use of very impure chloroform, such as even at a recent period was largely sold in London; and that, if pure chloroform alone had been employed, there would, by this time, have been no prejudice against its use. It is not, as I have shewn, necessary that chloroform should be very impure, in order to produce very disagreeable or even dangerous results. It is evident that even a small proportion of the oils above mentioned, if they are deleterious (and this cannot, I think, be doubted), will suffice, when applied in the form of vapour to the internal surface of the lungs, to act powerfully on the system. On the other hand, I am far from blaming those chemists who have manufactured impure chloroform for anything more than a want of due care in the preparation of an agent so energetic. And it is but fair to bear in mind that it was a new manufacture, hardly yet fully understood, and that those who made it were not probably aware, either of the existence of the impurities, or of the best mode of removing them. I have no doubt they did their best to produce a good article; and my chief object in this paper has been to put it in the power of every one to do so, and to point out strongly the bad effects of even a small amount of impurity.

While I acquit the makers of impure chloroform of any desire to adulterate it, I think it right to add that some of them must have been entirely ignorant of what was published concerning its properties. Thus some sold it of specific gravity 1.465, others of 1.347; and in the case of No. 8, which I have no doubt was under 1.000, although I had not enough to take its density accurately, the maker had evidently rejected the chloroform, and preserved the lighter liquid floating over it!—not knowing even that chloroform was a heavy liquid. It is lamentable to think that persons so ignorant are free, by our laws, to set up as makers of the most potent drugs.

I may here add, that no *rectification* at all is required from the first, if the chloroform be only washed with water till its volume no longer diminishes, and then treated, as above, with concentrated sulphuric acid.

It is possible that some of the fatal cases may have occurred from an injudicious mode of administering the vapour, or from the operator intrusting the administration to persons not qualified to recognise those signs which tell the experienced practitioner that it is time to stop. There ought always to be two well-qualified persons present,—one to watch, without intermission, the effects of the vapour, which he also administers as required; the other, of course, to operate. He who gives the chloroform must carefully attend to the state of the respiration, as has been often recommended by Dr Simpson. But these are matters beyond the proper province of this paper, and I leave them to those who are better qualified than I am to discuss them.

I have only to add, that this paper was written and read before I heard of a recent article in "Chambers' Journal" on the subject; and that I had not the remotest knowledge of or concern in that article, which I have not yet seen, although, as I am told, the author of it agrees with some of my conclusions in regard to the employment of chloroform in London.

A tabular view of the properties of chloroform will be found on the following page.

Tabular View of the Properties of Chloroform.

Variety of Chloroform.	Specific Gravity at 60°.	Action of Concentrated Sulphuric Acid.	When evaporated on hand.	GENERAL REMARKS.
No. 1	1·347	Became milky and yellow, changing to brown. After twenty-four hours, very dark brown; it also diminished in volume.	Left a strong smell.	The low density here at once proves the great impurity. In contact with the acid, it lost one-fifth or one-fourth of its volume. Very dangerous to use.
No. 2	1·465	The same as No. 1, except that it did not diminish nearly so much; after 24 hours, it had become very dark.	The same as No. 1.	Density also far too low, but less impure than No. 1. Both would be very unsafe to use.
No. 3	1·495	Scarcely affected, on mixture at first; after some time very dark, it also acquired a distinct smell of musk.	Left a very distinct smell.	This is much better than Nos. 1 or 2, but yet not pure.
No. 4	1·475	Became milky and thin yellow; after 24 hours, very dark brown.	The same as No. 1.	Resembling No. 2; probably the same maker.
No. 5	1·495	Became milky yellow, and afterwards brown, but diminished only slightly in volume; darker after 24 hours.	Distinct smell.	Of tolerable quality, but not pure.
No. 6	1·490	Became slightly yellow, but did not diminish much in volume; brown after 24 hours.	Very distinct smell.	Nearly as No. 5.
No. 7	1·495	Little colour developed at first; after frequent shaking and 24 hours contact, dark brown.	Distinct smell.	Rather better than Nos. 5 and 6. The chloroform mentioned in the note, p. 320, was not, I believe, inferior to this.
No. 8		Became dark brown, and very hot. Nearly the whole of it dissolved in the oil of vitriol used.	Left a very strong and disagreeable smell.	This certainly did not contain more than one-thirtieth of chloroform. It had not even the smell of that substance, and contained much free hydrochloric acid, as well as the poisonous oils, in large proportion. The use would be most dangerous.
No. 9	1·500	Became very pale yellow, afterwards dark brown.	Distinct smell.	The full density, and very nearly pure. Quite fit for ordinary use; although it might easily be rendered quite pure.
No. 10	1·500	Very slight change.	Just perceptible smell.	Full density. It can hardly be distinguished from the purest chloroform I have myself prepared. But even this did not exhibit the convex surface downwards, when resting on the acid.
No. 11	1·490	Became dark brown after a time, as No. 4.	Distinct smell.	Not sufficiently pure for use; but better than several others.
No. 12	1·490	As No. 9. Slightly coloured at first; after some time, and frequent shaking, dark brown.	Distinct but slight smell.	This, as well as Nos. 3, 5, 6, 7, and 11, would all have been called quite pure two years, or even one year ago. But all of these require to be purified.

The following Donations to the Library were announced :

Some Account of the last Yellow Fever Epidemic of British Guiana.

By Daniel Blair, M.D. Edited by John Davy, M.D., F.R.S.L. & E.

8vo.—*By the Author.*

Das periphere Nervensystem der Fische, Anatomisch und Physiologisch untersucht von Dr Hermann Stannius. 4to.—*By the*

Author.

Neue Denkschriften der Allg. Schweizerischen Gesellschaft für die gesammten naturwissenschaften. Bd. x., mit. xiii. Tafeln. 4to.

—*By the Society.*

On the Diffusion of Liquids. By Thomas Graham, Esq., F.R.S.,

F.C.P. 4to.—*By the Author.*

Description of the Instruments and Process used in the Photographic Self-registration of the Magnetical and Meteorological Instruments at the Royal Observatory, Greenwich. 4to.—*By the*

Astronomer-Royal.

Proceedings of the Royal Astronomical Society. Vol. X., No. 4.

8vo.—*By the Society.*

Description of the Observatory at Cambridge, Massachusetts. By

William Cranch Bond. 4to.

Astronomical Observations made at Cambridge Observatory, Massachusetts, 1847–8. 8vo.—*By the Observatory.*

Monday, 1st April, 1850.

Gen. Sir T. MAKDOUGALL BRISBANE, Bart., in
the Chair.

4. On a Peruvian Musical Instrument, like the ancient
Syrinx. By Dr Traill.

The author prefaced his description of the instrument, by a few general remarks on the communication, in very remote epochs, between the inhabitants of the old and new worlds, as deducible from affinities in their traditions, their cosmogenies, their religious rites and structures, their astronomical cycles, and their determination of the length of the year.

The Peruvian instrument was discovered, some years ago, in a *huaco*, or vast tumulus, that was believed to cover the remains of an Inca of Peru. It is not of unequal reeds, like the Greek *syrinx*, but is cut out of a piece of *potstone*, of a trapezoidal form, in which are cut eight tubular holes of unequal depths. These tubes or holes are of equal diameter, and have been carefully made with some sort of drill. The breadth of the instrument, including a short handle, is 6.2 inches; its greatest depth, 5.3 inches; and the thickness of the stone varies from 0.7 to 0.5 inch. The instrument in principle and in form is analogous to the *Pan's pipe* of antiquity, or to the *organetto* of modern Italy; but has one remarkable difference in a small ventilage on each of four of its pipes; when one is uncovered, that pipe is mute, but when covered by the fingers of the player, the full sound is produced.

A strolling Italian, who performed well on the *organetto*, was employed for several evenings to play on the Peruvian instrument; and, with the assistance of three skilful musical friends, one of whom was an adept on the violoncello, the author of the paper was enabled to ascertain the scale of the instrument. This scale extended from E on the lower line, through F sharp, G, A, D, C sharp, F to A, above the lines. By means of the ventilages, the ordinary notes of the instrument seemed to be divisible into two tetrachords,—one in the key of E minor, the other of F major—the first a perfect tetrachord; the second, nearly so.

The form of the instrument and its use have a striking similarity to the *Syrinx* of the Greeks, the invention of which was ascribed to the god Pan, or to Egypt; and it is worthy of notice, that the great musical system of the Greeks also consisted of tetrachords. A *syrinx* of unequal reeds was found by the celebrated Humboldt, in the hands of the natives, on the banks of the Orinocco. It is in use among the Arabs of the desert, and a similar instrument, composed of twelve unequal reeds, is figured by Kämpfer among the instruments of the Japanese.

2. Some Remarks on Cometary Physics. By Professor Piazzì Smyth.

That theories of the physical appearances of comets have generally failed, appeared to the author to arise from the facts having been misunderstood or misinterpreted in general by the observers themselves.

As a particular instance of this, the wide-spread notion of comets shooting forth their tails, at, or a little before the perihelion passage, and drawing them in again afterwards, so as to be larger at that period of their orbits than at any other, was mentioned; and in place of which, the author shewed that the comets were at the perihelion, of their smallest size; the tails becoming then more visible, not from being actually produced at that time, but from being more dense, and illumined by a stronger solar light, as well as being in general seen from a smaller terrestrial distance.

The author then proceeded to collect together the facts which he thought well made out with regard to comets; to describe the corrections which the apparent, required, to give the true phenomena; and to detail the various practical methods by which better observations might be procured.

The so-called established facts mentioned above, were collected in a series of axioms, which are here appended; as they seem to be worthy of being discussed, and either disproved or assented to, by astronomers.

1st, A comet consists of a nucleus, and one or more gaseous envelopes.

2d, The nucleus, if solid and material, is infinitely small.

3d, The nucleus is excentrically situated in the gaseous body.

4th, Comets of longest period have the largest bodies.

5th, Those comets whose orbits have the greatest excentricity, are the most excentrically situated in their envelopes, or, vulgarly, have the longest tails.

6th, A comet revolves on an axis passing through the nucleus, and at right angles to the major axis of the envelope, in the same period of time that it takes to revolve about the sun; hence the tail being turned away from the sun in the normal position, is turned away from him in all other parts of the orbit also.

7th, This axis is not at right angles to the plane of the orbit, but variously inclined in the case of different comets, as with the planets.

8th, A quicker rotation round the longer axis of the body also appears to exist.

9th, A comet shines by reflected light, and shews a sensible phase.

10th, The gaseous envelope is of extreme tenuity, is elastic, and,

with regard to light, is slightly reflective and imperfectly transparent ; it decreases in size, but increases in density and light reflective power in approaching the perihelion, and the reverse when receding from it ; and this occurs in a degree proportioned to the excentricity of the orbits of the comets.

11th, The axis of the tail of a comet is straight at the perihelion, but at any point between this and the aphelion, is curved ; and is concave towards the latter, the radius of curvature being inversely as the excentricity of the orbit.

12th, The molecules composing the envelope of a comet are only held together by their mutual gravitation, each constituting almost a separate independent projectile, and describing its own parabola about the sun.

3. Abstract of Professor Kelland's Exposition of the Views of D. R. Hay, Esq., on Symmetric Proportion.

The fundamental hypothesis of the author was stated to be this :— That the eye is capable of appreciating the exact subdivision of spaces, just as the ear is capable of appreciating the exact subdivisions of intervals of time ; so that the division of space into an exact number of equal parts will affect the eye agreeably in the same way that the division of the time of vibration in music, into an exact number of equal parts, agreeably affects the ear. But the question now arises, What spaces does the eye most readily divide ? It was stated that the author supposes those spaces to be angles, not lines ; believing that the eye is more affected by direction than by distance. The basis of his theory, accordingly, is, that bodies are agreeable to the eye, so far as symmetry is concerned, whenever the principal angles are exact submultiples of some common fundamental angle. According to this theory we should expect to find, that spaces, in which the prominent lines are horizontal and vertical lines, will be agreeable to the eye, when all the principal parallelograms fulfil the condition that the diagonals make with the sides, angles which are exact submultiples of one or of a few right angles. This application of the theory was exemplified by a sketch of the new Corn Exchange erected in the Grassmarket by David Cousin Esq., whose beautiful design was shewn to have been constructed with a special reference to the fulfilment of this condition.

The author was stated to proceed to apply his theory to the con-

struction of the human figure, in which we should expect *à priori* to find the most perfect development of symmetric beauty. Diagrams were exhibited which represent, with remarkable accuracy, the human figure; and it was explained that not a single lineal measure is employed in their construction. The line which shall represent the height of the figure being once assumed, every other line is determined by means of angles alone. For the female figure, those angles are, one-half, one-third, one-fourth, one-fifth, one-sixth, one-seventh, and one-eighth of a right angle, and no others. It must be evident, therefore, that, admitting the supposition that the eye appreciates and approves of the equal division of the space about a point, this figure is the most perfect which can be conceived. Every line makes with every other line a good angle. The male figure was stated to be constructed upon the female figure by altering most of the angles in the proportion of 9 : 8; the proportion which the ordinary untempered flat seventh bears to the tonic.

A drawing was exhibited, which had been designed with great care from the life, by the distinguished academician John A. Houston, Esq. On this drawing the author had constructed his diagrams; and the coincidence of theory with fact was seen to be complete. Professor Kelland concluded by claiming for the author the attention of the Society. He argued, that a principle so simple and comprehensive in its character, and thus far apparently truthful in the conclusions to which it leads, merits, and should receive, the most complete and rigid examination. Whatever might be the ultimate result (and it promised to be satisfactory in the extreme), the ingenuity, energy, and zeal, shewn by the author, entitle him to our warm approbation.

The following Donations to the Library were announced :

Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, 1847. 4to.—*From the Observatory.*

Journal of the Statistical Society of London. Vol. XIII., Part 1, 8vo.—*By the Society.*

Deuxième Mémoire sur le Daltonisme, ou la Dyschromatopsie, par E. Wartmann. 4to.—*By the Author.*

The Accommodation of the Eye to Distances. By William Clay Wallace, M.D. 8vo.—*By the Author.*

Transactions of the Zoological Soc. of Lond. Vol. III., Pts. 5 & 6. 4to.
 Proceedings of Do. Parts 15 & 16. 8vo.
 Reports of Council of Do. 1849. 8vo.—*By the Society.*

Monday 15th April.

Rev. Dr GORDON in the Chair.

The following Communications were read:—

1. On the Constitution of Codeine, and its Products of Decomposition. By Thomas Anderson, M.D.

The author commenced his paper by referring to the analysis of codeine made by different chemists. On these analyses four different formulæ had been founded; but two only, those of Regnault and of Gerhardt, required special mention, the others being now known certainly not to represent the constitution of the base. Regnault had deduced from his analysis the formula $C_{35}H_{20}NO_5$, while Gerhardt gives $C_{36}H_{21}NO_6$ as the expression of his results.

The author submitted codeine to careful analysis, and obtained the following results:—

					Calculation.
Carbon,	71.91	72.02	72.09	72.09	72.24
Hydrogen,	7.05	7.04	7.14	7.16	7.02
Nitrogen,	4.41	4.60	4.50	...	4.68
Oxygen,	16.63	16.34	16.27	...	16.06
	<hr/>	<hr/>	<hr/>	<hr/>	<hr/>
	100.00	100.00	100.00		100.00

agreeing closely with the formula $C_{36}H_{21}NO_6$, and confirmed by the analysis of its platinum salt, which contains an equivalent of water, and gave, as the mean of seven experiments, 19.25 per cent. of platinum, while the calculated quantity is 19.19 per cent.

The author then describes in detail the properties and constitution of its salts. The hydrochlorate crystallizes in groups of short radiated needles, the formula of which is $C_{36}H_{21}NO_6HCl + 4HO$. The hydriodate is obtained in long needles, which, dried at 212° , retain two equivalents of water, and have the formula $C_{36}H_{21}NO_6HI + 2HO$. The sulphate, nitrate, phosphate, oxalate, hydrosulphocyanate, and platinochloride are also described.

The author then proceeds to the consideration of the products of decomposition of codeine.

When treated with strong sulphuric acid, codeine passes into an amorphous condition, similar to that in which quinine is obtained when treated with an excess of acid, and in which state it forms resinous compounds with acids.

With dilute nitric acid it gives a new base, nitrocodeine, the formula of which is $C_{36} H_{20} (NO_4) NO_6$, which is precipitated from its solution by ammonia, in minute silvery crystals, sparingly soluble in water, but dissolving readily in alcohol and ether; and crystallising on cooling in small yellowish needles. It dissolves readily in acids, with the formation of salts, which have a more or less yellow colour; and all crystallize except the hydrochlorate. Of these the hydrochlorate, sulphate, oxalate, and platinochloride are described.

By the action of bromine, two different bases are obtained—bromocodeine and tribromocodeine. The first of these is prepared by adding bromine water to powdered codeine until it is dissolved, and then precipitating with ammonia, when the base is thrown down as a crystalline powder, which is obtained in needles by solution in boiling water or alcohol. Its formula in the crystallized state is $C_{36} H_{20} Br NO_6 + 3 HO$. Its salts are similar, in most of their properties, to those of codeine, and all crystallize in small needles. By the further action of bromine, a yellow powder, sparingly soluble in water, is obtained, which is the hydrobromate of tribromocodeine, and from which the base is obtained by solution in hydrochloric acid, and the addition of ammonia. Tribromocodeine is a gray powder, insoluble in water and ether, but soluble in alcohol; it is an extremely feeble base, but dissolves in acids and forms salts, all of which are sparingly soluble in water and amorphous. Its formula is $C_{36} H_{18} Br_3 NO_6$.

The author found that chlorine, by acting upon codeine, gave rise to amorphous compounds, which were not obtained of definite constitution; but by the use of a mixture of chlorate of potash and hydrochloric acid he obtained chlorocodeine, $C_{36} H_{20} Cl NO_6$, similar in its general properties and constitution to bromocodeine, and resembling that substance so closely that it may be easily mistaken for it.

By the action of cyanogen another base was obtained. This

substance is best prepared by passing cyanogen into codeine dissolved in the smallest possible quantity of alcohol. The gas is rapidly absorbed, and there is deposited from the solution a mass of crystals which, when dissolved in alcohol, are obtained in six-sided plates, with a fine silvery lustre. These crystals gave to analysis the following results:—

Carbon,	68·22	68·04
Hydrogen,	5·93	6·17
Nitrogen,	11·81	11·50
Oxygen,	14·04	14·27

and the author attributes to them the formula $C_{36}H_{21}NO_6 \cdot 2C_2N$, and gives to the substance the name of bicyanocodeine. It is a base; but owing to its extreme instability, no salts could be obtained. When treated with an acid it is rapidly decomposed, ammonia being formed, and, after a time, hydrocyanic acid evolved.

By treating codeine with a mixture of potash and lime, at a temperature of 250° Fahr., it undergoes slow decomposition, and a volatile base is evolved, which differs according to the circumstances of the experiment. The author found that, under certain circumstances, the base evolved had the formula C_6H_9N , and forms the term in the series of bases homologous with ammonia, which corresponds to metacetic acid, and which may be called metacetamine. Under other circumstances the base evolved had the formula C_2H_5N , and corresponded, in all its properties, with the methylamine of Wurtz.

The following is a tabular view of the constitutions of the substances described in this paper:—

Codeine,	. . .	$C_{36}H_{21}NO_6$.
... crystallised,	. . .	$C_{36}H_{21}NO_6 + 2HO$.
Hydrochlorate,	. . .	$C_{36}H_{21}NO_6 HCl + 4HO$.
Hydriodate,	. . .	$C_{36}H_{21}NO_6 HI + 2HO$.
Sulphate,	. . .	$C_{36}H_{21}NO_6 HO SO_3 + 5HO$.
Nitrate,	. . .	$C_{36}H_{21}NO_6 HO NO_5$.
Phosphate,	. . .	$(C_{36}H_{21}NO_6 HO) 2HO PO_5 + 3HO$.
Oxalate,	. . .	$C_{36}H_{21}NO_6 HO C_2O_3 + 3HO$.
Hydrosulphocyanate,	. . .	$C_{36}H_{21}NO_6 HC_2NS_2 + HO$.
Platinum salt dried at 212° ,	}	$C_{36}H_{21}NO_6 HCl Pt Cl_2 + HO$.
... crystallised,		$C_{36}H_{21}NO_6 HCl Pt Cl_2 + 3HO$.

Amorphous codeine,	. $C_{36} H_{21} NO_6$.
Nitrocodeine,	. $C_{36} H_{20} (NO_4) NO_6$.
Sulphate,	. $C_{36} H_{20} (NO_4) NO_6 HO SO_3$.
Platinum salt,	. $C_{36} H_{20} (NO_4) NO_6 HCl Pt Cl_2 + 4 HO$.
Bromocodeine,	. $C_{36} H_{20} Br NO_6$.
... hydrate,	. $C_{36} H_{20} Br NO_6 + HO$.
... terhydrate,	. $C_{36} H_{20} Br NO_6 + 3 HO$.
Hydrobromate,	. $C_{36} H_{20} Br NO_6 HBr + 2 HO$.
Platinum salt,	. $C_{36} H_{20} Br NO_6 HCl Pt Cl_2$.
Tribromocodeine,	. $C_{36} H_{18} Br_3 NO_6$.
Hydrobromate,	. $2 (C_{36} H_{18} Br_3 NO_6) 3 HBr$.
Platinum salt,	. $C_{36} H_{18} Br_3 NO_6 HCl Pt Cl_2$.
Chlorocodeine,	. $C_{36} H_{20} Cl NO_6$.
... terhydrate,	. $C_{36} H_{20} Cl NO_6 + 3 HO$.
Sulphate,	. $C_{36} H_{20} Cl NO_6 HO SO_3 + 4 HO$.
Platinum salt,	. $C_{36} H_{20} Cl NO_6 HCl Pt Cl_2$.
Bicyanocodeine,	. $C_{36} H_{21} NO_6 2 C_2 N$.
Metacetamine,	. $C_6 H_9 N$.

2. On the *Physical* and Scottish *Statutory* Limits of Sea and River, as applicable to Salmon Fisheries. By Dr Fleming.

Dr Fleming directed the attention of the Society, in the first instance, to the characteristic features of *sea* and *river* proper; and then proceeded to consider the peculiarities of that *common space*, alternately sea and river, to which he restricted the term *estuary*. He then considered the nature of the space between high and low water, and pointed out the *mean level*, or mid-tide mark, as the only constant and universally applicable boundary plane. The influence of the tidal wave in reversing the current, checking the velocity, and increasing the depth of the river, was next brought under notice, and an experiment exhibited, illustrating the conservation of force, which causes the waters at the head of an estuary, and the connected river, in certain circumstances, to attain a *higher level* than the high-water mark of the neighbouring sea-shore. He then considered, successively, the tests which, on different occasions, had been proposed and employed; viz.—point of stagnation; presence of sea or river water; the growth of sea-weeds; fauces terræ; deltas and bars; and pointed out their uselessness in determining the physical limit between sea and river.

The second part of the paper was occupied with an examination of the Scottish *statutory* limit of sea and river, as applicable to the salmon fisheries ; in which the author indicated *low-water mark*, as *the only limit contemplated*, and justified the sagacity of our ancient legislators, by proving that, with this limit, the object of the statutes was secured. He pointed out the inapplicability of the *physical* test which he had previously established, and of the spurious ones which had been noticed, to the settlement of the fishery question. He concluded, by expressing his regret, that the Legislature had declared certain engines, for catching fish, to be legal or illegal, according as they are used in sea or river, without defining what is *sea* or what is *river* ; and his expectation that, should any bill be brought into Parliament, in connection with this subject, the present state of the law will not be permitted to remain in *culpable obscurity*.

3. On the Combined Motions of the Magnetic Needle, and on the Aurora Borealis. By J. A. Broun, Esq. Communicated by Sir T. M. Brisbane, Bart.

When a steel needle or rod is so constructed that its centre of gravity is in a finely-turned axle at right angles to its length, it will rest in any position when the axle is placed upon polished planes ; when, however, we magnetize the needle, it assumes a position which is that of the direction of the magnetic force at the place : in this way we obtain the ordinary dipping-needle. The dipping-needle can obviously move only in one plane, that to which the axle is at right angles ; were it possible to suspend it freely, so that it could move in every plane with every variation of the direction of the magnetic force, we should then be able, by observing the variations of its position, to determine at once the laws which a magnet in its true position obeys ; this, however, we have not been able to do ; even the small variations in the vertical plane, which we might expect to obtain from the ordinary dipping-needle, are nearly or altogether destroyed by the friction of the axle upon its supports ; and there are many mechanical difficulties in the way of the other methods of suspension. It has been found convenient, then, to make use of the simplest methods of suspending magnets in a horizontal plane ; and to endeavour to deduce, from the composition of their motions, the

laws both of the variation of the force with which a truly suspended magnet is directed, and of the direction of that force itself.

The most convenient of these is that termed the declination magnet, which is suspended horizontally by a fine silken thread; the tendency of the needle to dip being obviated by placing the point of suspension north of the centre of gravity. This instrument is very convenient, especially in high latitudes, for exhibiting in a magnified form that portion of the motion of the freely suspended dipping-needle, which is at right angles to the vertical plane of the needle. Two other instruments, one termed the bifilar magnetometer, from its suspension by two threads; the other named the balance magnetometer, from its resemblance to the beam of a balance, enable us to observe the variations of the horizontal and vertical components of the force with which the freely suspended dipping-needle is directed; whether these variations be due to a change in the total value of the force, or simply to a change in its direction parallel to the vertical plane. In high magnetic latitudes, the bifilar or horizontal component magnetometer will be most affected by changes of the direction of the force in the vertical plane, and the balance or vertical component magnetometer will be most affected by variations of the intensity of force: in low latitudes the reverse is the case. In all three instruments the magnets are forced from their natural position. By means of a well-known formula, however, we can compute, from the observed variations of the two components, the variations of the total force, and of its direction in the plane of the magnetic meridian. Theoretically this operation is simple enough, but practically there are great difficulties; these difficulties are due to the effect of temperature upon the positions of the bifilar and balance magnets, which require to be eliminated, and to sources of error that I have pointed out in the *Edinburgh Transactions* in the determinations of the change of value of either component of force, which corresponds to a change of, say one minute in the angular positions of the magnets. I conceive that I have, by the employment of new methods, reduced the errors due to these causes to a very small amount; and it is for this reason that I claim for the results deduced from the *Makerstoun Observations*, a consideration which they could not otherwise have been entitled to. I refer to the part of the *Transactions* now in the press, for the results relative to the *separate* magnetic elements, and to the total force; I confine myself at present to those touching the

motions of a magnet supposed freely suspended in the direction of the magnetic force.

I may state shortly the process by which the following results have been arrived at. The corrected observations for each of the three magnetometers having been discussed with reference to a particular argument; such as, the month, the moon's age, the moon's position in declination, the sun's hour angle, and the moon's hour angle; the motion of the (supposed) freely suspended needle at right angles to the plane of the magnetic meridian, was obtained with reference to the argument in multiplying the corresponding variations of declination by a constant factor (the cosine of the dip); the motion parallel to the same plane was obtained from the variations for the two components by the formula already referred to; the value of the former part of the motion for any epoch being taken as the abscissa, and that of the latter for the same epoch as the ordinate, the motion of the north end of the needle is constructed.

Annual Motions.—The difficulty of determining the law of annual variation of any of the magnetic elements has been so great, that it is doubtful whether that for the magnetic declination has ever been obtained, though the instrument upon which its determination depends is unaffected by variation of temperature. I believe that I have succeeded in the determination of the laws of all the elements, and from these the annual motion has been constructed. The annual motion deduced from the observations of the three magnetometers for the four years 1843, 1844, 1845, and 1846, is shewn in figure A; another and rather more symmetrical figure, deduced from a different combination of years, is shewn in figure B, Plate VI., Edin. Trans., Vol. xix., Part 2.

From near the vernal till the autumnal equinox the annual motion forms the half of an ellipse whose major axis, passing at the vertex through June, makes an angle of about $+11^{\circ}$ in figure A and of $+16^{\circ}$ in figure B with the projection of the magnetical meridian. At the autumnal equinox the north end of the needle again ascends till the winter solstice, after which it descends till the vernal equinox. In its descent, the north end of the needle having crossed its previously ascending path, it forms a loop which, when untwisted and continued downwards from the equinoxes, completes the ellipse; the portion formed by the loop having almost exactly the same perimeter as that regularly formed when the sun is north of the equator;

the completed portion is indicated by dotted lines in figures A and B. It does not seem improbable that in southern latitudes the figure will be inverted, and that it will be a simple ellipse near the equator.

Monthly Motions.—The motion corresponding to the moon's varying phase has not been projected, chiefly because of the irregularities still existing in the result of the four years' observations for the magnetic declination, the epoch of minimum being ill-determined; it is conceived that the figure is a simple ellipse with its major axis in the astronomical meridian, the northern extremity being at conjunction, the epoch of minimum dip, and the southern extremity at opposition, the epoch of maximum dip; this, however, is doubtful.

The motion for the moon's position in declination has been obtained in the following manner:—Having first projected the means of magnetic declination for *each* three days of the moon's position in declination, as obtained from the Tables for the years 1843–6, the day after the farthest northerly position being the abscissa, a curve was passed freely among the points; the values of the ordinates at the points of intersection by the curve were then taken as the interpolated value of magnetic declinations for the corresponding abscissæ: a similar operation was performed for the magnetic dip. In both cases very satisfactory curves, agreeing nearly with the true points, were obtained. These values are projected in figure C, Plate VI., Edin. Trans., Vol. xix., Part 2. From this figure the north end of the dipping-needle commences its ascent about two days after the moon is north of the equator, attains its highest point about two days after the moon is farthest north, and afterwards it descends till the moon is again near the equator; thus forming a figure like a portion of an ellipse with its vertex about one day after the moon is farthest north, the major axis making an angle of about -30° with the magnetic meridian. It will be remarked that so far this motion is quite similar to that for the sun's position in declination, with the exception of the axis of the figure being on the opposite side of the magnetic meridian; when we trace the figure farther, the analogy still subsists;—as the moon proceeds south of the equator the north end of the needle again ascends till the moon is farthest south, thereafter descending, and, in crossing its previously ascending path, a loop is formed lying partially out of the principal figure, as in the case of the annual motion.

The correspondence of the two results gives a great weight to the accuracy of both; this will be more evident when it is remembered, that the whole motion of the dipping-needle for the moon's varying declination is included by a small circle with a diameter of little more than *one-tenth of a minute of space*, and, that no observation in the sixty thousand employed for this result has been rejected, however greatly affected by disturbance; although the graphic interpolation to remove slight irregularities may be considered as an equivalent operation.

Diurnal Motions.—The monthly mean diurnal variations for the magnetic declination and magnetic dip, from four years' observations, still present irregularities, especially from 10^h P.M. till 4^h A.M., the hourly positions for this time depending on only two years' observations. For this reason, the values from the Tables having been projected, curves were passed freely among the points, and the interpolated ordinates thus formed, were taken for the projections in Plate VII.: the interpolated quantities differ very little from the actual values, and this is especially the case for the summer months.

The diurnal motions for the four winter months, November to February, are of the same class, and they differ considerably from those for the other months (see Plate VII.): in each of these months the motion consists of a figure of two closed loops: the north end of the needle moves eastwards with little change of dip from about 1^h P.M. till 9^h or 10^h P.M., after which it turns westwards, and begins to ascend about 4^h A.M., crossing near its position at 6^h P.M.; thus forming an eastern loop, which is small compared with the western loop, excepting in December. After 6^h A.M., the north end of the needle having moved a little westwards, again descends, crossing a second time the afternoon track near 5^h P.M.; still moving westwards, it ascends about 11^h A.M. till it meets the position of 1^h P.M., thus completing the western loop. The eastern loop is not formed in March, the north end of the needle not rising sufficiently high to cross the afternoon track. The change in the figure from February to March is very great; in April and May the remains of the eastern loop are still visible, but in June and July its position is indicated by a simple inflection in the figure; in August and September the germ of the eastern loop becomes more distinct, and in October the loop is actually formed. The transition in form from autumn to winter is quite gradual, unlike that from winter to spring. In the

winter months, the principal or western loop is formed by the motion from 8^h A.M. till 5^h P.M. ; in the months from April to August, three-fourths of the whole diurnal motion occur between 6^h A.M. and 6^h P.M., the remaining fourth forming a slightly inflected side to each of the figures : it is this side which is gradually twisted up to form the eastern loop of the winter months.

It is evident that no proper comparison can be made of the areas of these figures on account of the involved forms in the winter months ; the areas, however, of the figures from April to August, differ very little.

Perimeters of the Figures.—The twisting of the perimeters, which renders a comparison of the areas of little value, does not appear to affect the length of the motion, and this therefore seems a fair subject for examination. The following are the values of the angular motion, or length of the perimeter, for each month, as obtained approximately from Plate VII.

Jan.	Feb.	March.	April.	May.	June.
5'60	6'16	9'22	12'18	12'04	12'00
July.	Aug.	Sept.	Oct.	Nov.	Dec.
11'56	11'64	10'48	9'78	7'22	5'84

December and January shew the least perimeters, April, May, and June, the greatest, though the perimeters for the months from April to August are nearly constant.

Hourly Angular Motions.—Having obtained the approximate motion from hour to hour for each of the monthly figures, we find that, on the whole, they follow nearly the same law, that indicated in the following numbers, which are the means for each two hours of the *hourly* motions from the 12 separate months.

12 ^h	14 ^h	16 ^h	18 ^h	20 ^h	22 ^h	0 ^h	2 ^h	4 ^h	6 ^h	8 ^h	10 ^h
0'43	0'48	0'46	0'62	1'19	1'60	1'34	1'08	0'99	0'60	0'57	0'29

These numbers give the following curious result ;—That the velocity of motion of the north end of a magnet freely suspended in the direction of the magnetic force is a maximum when the sun makes its superior transit of the magnetic meridian (between 10^h and 11^h A.M.), and a minimum when it makes its inferior transit of the same meridian (between 10^h and 11^h P.M.). This result is the more curious that the epoch of the minimum velocity of the diurnal motion is an epoch of maximum disturbance ; and, in as far as the declination is con-

cerned, the epoch of maximum velocity of the diurnal motion is also an epoch of minimum disturbance.

When we compare the results for the irregular disturbance, with reference to the separate elements of magnetic declination and magnetic dip (see Ed. Trans., Vol. xix., Part 2), with the velocities of motion as deduced from these figures, we find, that *when the diurnal motion is most rapid the departures from the direction of that motion are least, and when the diurnal motion is slowest the irregular departures from the hourly mean position are greatest.*

Thus, if we examine the mean disturbance of magnetic declination for each hour, as deduced from two years' observations, we find it a maximum during the hours from 8 P.M. till 2 A.M.; this is the period for which the motion of the needle is at once slowest and least as regards the declination; about 21^h (referring to the figure for the year, see Plate VIII., Edin. Trans., Vol. xix., Part 2), the motion is most rapid and nearly altogether in declination, the minimum disturbance in declination occurs immediately before this hour; another and nearly equal minimum occurs under the analogous circumstances about 5^h P.M.; a secondary maximum occurring about 1^h or 2^h P.M.

If we approximate to the hourly mean disturbance of the magnetic dip by means of those deduced for the two components of force, we find the *minimum* to occur about 6^h–7^h A.M., when the velocity of motion is considerable, and when almost wholly in the *direction* of dip; the disturbance increases from that time till about 2^h A.M., shewing a secondary minimum about 1^h P.M. and about 8^h P.M., at both of which times the direction of motion is chiefly that of dip: the maximum disturbance occurs from about 10^h P.M. till 3^h A.M., during which period the velocity of motion is least.

On the whole, then, the magnetic disturbance appears to be chiefly at right angles to the direction of the motion of the needle, and to be inversely as the velocity of motion.

It is scarcely possible to connect the previous facts of area, perimeter, or velocity of motion with the laws of variation of temperature. In the mean for the whole year, the temperature changes most rapidly between 8^h and 9^h A.M.; but it changes with nearly equal rapidity between 5^h and 6^h P.M. There is no corresponding fact in the previous numbers. When we compare the variations of temperature with the variations of position for the suspended mag-

net in the summer months, we find the difference between the two classes of facts even more marked: in summer, the temperature changes most rapidly about 7^h A.M. and 7^h P.M., the change for May, June, and July, from 6^h–8^h A.M., being $+ 3^{\circ}80$, and from 6^h–8^h P.M., being $- 3^{\circ}54$; for the same months, the mean angular motion of the needle from 6^h–8^h A.M. = $1^{\circ}00$, from 9^h–11^h A.M. = $2^{\circ}12$, and from 6^h–8^h P.M. = $0^{\circ}74$. There is a diminution in the velocity of the motion between 1^h and 2^h P.M.; there is also a slight diminution at the turning point, 6^h–7^h P.M., and between 2^h and 3^h A.M. These diminutions appear to be connected with the fact, that they occur at turning points in the figures.

It may be remarked that the line representing the astronomical meridian, and passing through the centre of gravity of the perimeters of the figures, for the months during which the sun is north of the equator, also passes through the position of greatest velocity, and nearly through that of least velocity, of the diurnal motion.

General Form and Turning Points of the Diurnal Motions.
—The general forms of the diurnal motion vary between rude ellipses and circles. In the winter months, the principal portion, or loop of the figures, is elliptical, with the major axis horizontal; near the equinoxes, the figure becomes somewhat circular, and in the midsummer months it again becomes rudely elliptical, with the major axis inclined about 20° or 30° west of the magnetic meridian. In the usual investigations of the conventional element of declination, it has been remarked, that the turning from the farthest westerly position occurs near the time of maximum temperature; a coincidence which has been supposed to indicate a real connection, though there is no similar coincidence between the epoch of minimum temperature, and the eastern turning point. If, however, we examine the figures indicating the diurnal motions of a needle in its *true* position, such as those for the months of April, August, October, &c., we might find it difficult to say where is a turning point and where not; and it is difficult to see why the turning points at the extremities of the horizontal diameters of these rude circles, or at the extremities of a horizontal line, in the ruder ellipses, should be chosen, in preference to the turning points at the extremities of other lines drawn in the figures, as tests for a theory; unless, indeed, it be explained by the accident that a horizontal suspension of a magnetic needle, is a convenient one for observing a certain portion of the

motion of a magnet, which, independently of gravity, would rest in the direction of the magnetic force.

It has been customary, however, to give theories of the cause of magnetical variations, with reference solely to the diurnal variations of the magnetic declination (and not unfrequently with a very indifferent knowledge of the facts with respect even to that element). I venture to say, that it will only be from a careful comparison of the whole facts relating to the motions of a freely suspended dipping needle, not for one place, but for different and distant portions of the earth's surface, that a satisfactory theory will be obtained. The attempt to deduce one from a consideration of the declination variations alone, can only be likened to a similar attempt with reference to planetary motions, the apparent position of the planet being studied without any relation to the direction or rate of motion of the place of observation.

Dr Lloyd, who has done so much for magnetical science, has lately brought forward a discussion of his declination observations, which he considers strongly in favour of the theory that the diurnal variations of magnetic declination are due to the sun's heating effect upon the earth, in opposition to the atmosphere. I venture also to offer my guess, founded upon a consideration of various meteorological facts, that it is in the atmosphere, and not the earth, that we shall find seated the secondary causes of magnetic variations. In the meantime, it is *facts* that are wanted.

It may be noticed, chiefly with reference to the months from March to October, that a line passing through the positions of noon and midnight, also passes through, or nearly through, the mean position, or the centre of gravity, each hour having equal weight: also a line passing through the positions, about four hours before, and four hours after noon, passes nearly through the centre of gravity of the perimeters; the former of these lines lies nearly in the direction of the minor axis, the latter nearly in that of the major axis of the rude ellipses for the midsummer months. The horizontal line passing through the centre of gravity also passes nearly through the positions of 1^h A.M. and 1^h P.M., which, therefore are the epochs of mean dip.

Angular Distances between the Hourly Positions from the Mean of all, and from the Undisturbed Days.—In order to render the following result intelligible, it must be stated that, after a careful

examination of each day's observations in the years 1844 and 1845, a series was selected, in each month, of days nearly unaffected by magnetic irregularity; the diurnal variation was then obtained for these undisturbed days, and this was compared with the diurnal variation deduced from all the observations; the assumption being made that the mean for the whole 24 hours was unaffected by disturbance, the differences of the hourly values would evidently shew the effect of disturbance on the *hourly* mean position. This assumption, it was found, must be as nearly as possible true for the magnetic declination, because the monthly means of the selected days differed little or nothing from those of all the days; this, however, is not the case for the element of dip, the disturbance appeared to affect the daily or monthly mean to a small extent. Confining myself here to the result for the year (referring to the volume of the Transactions for the partial results which vary with season); the following numbers indicate the displacement of the mean hourly positions by disturbance, upon the assumption that the centre of gravity for each figure is the same:—

12 ^h	14 ^h	16 ^h	18 ^h	20 ^h	22 ^h	0 ^h	2 ^h	4 ^h	6 ^h	8 ^h	10 ^h	12 ^h
0·35	0·25	0·06	0·15	0·27	0·30	0·23	0·31	0·30	0·17	0·31	0·39	0·35

The diameter of the figure is little greater than 2'·0.

In the mean figure for the year (see Plate VIII. already referred to), minima occur at 4^h A.M. and about 5½^h P.M., the maximum occurs about 10^h P.M., and a maximum occurs between 8^h A.M. and 4^h P.M. If, making allowance for the effect of disturbance on the position of the centre of gravity with reference to dip, we suppose the centre of gravity of the dotted figure for the year, raised 0·15 on the line of mean declination, or that of the continuous figures lowered as much, we find the maximum effect of disturbance to occur about 10^h P.M. and 10^h A.M., and the minimum effect about 4^h A.M. and 5^h P.M. This result was obtained for the magnetic declination in 1844, and is given in the volume for that year.

Motions with reference to the Moon's Hour-Angle.—These, as obtained from the means of all the lunations in the years 1844 and 1845, and as deduced from winter lunations for 1845 only, are shewn in Plate VII. The resulting figures, especially that for the winter lunations of 1845, bear some resemblance to the diurnal motion for the month of December.

AURORA BOREALIS.

A table of 184 auroræ seen at Makerstoun in years 1843 to 1849 is given in pages lxxv.-lxxviii. of Vol. xix., Part 2; from this table the following results have been obtained :—

A very careful outlook for auroræ was kept throughout the whole period, but especially during the first five years; an outlook warned by magnetic disturbance in circumstances unfavourable to the visibility of the meteor, and assisted by a practical acquaintance with the faintest auroral indications. In several cases, the auroral appearances were very faint; these are entered in the table as "Traces," and, in others, there was doubt whether the appearance was truly auroral; these are indicated by "Trace?" It should be noted that, with the exception of the years 1844 and 1845, auroræ were seldom looked for after midnight.

Diurnal variation of frequency of the Aurora Borealis.—The following are the numbers of times which auroræ were seen, at each hour, from 5^h P.M. till 5^h A.M., for the whole period—referring to the printed tables for the numbers for each season.

Hour,	5 ^h	6 ^h	7 ^h	8 ^h	9 ^h	10 ^h	11 ^h	12 ^h	13 ^h	14 ^h	15 ^h	16 ^h	17 ^h
No.,	5	19	45	57	91	75	50	37	27	15	11	3	2

The greatest number of auroræ were seen at 9^h P.M.; this result is independent of the effect of twilight, since 9^h P.M. is also the hour of maximum frequency for the winter months. This hour is nearly the hour of maximum disturbance for the magnetic declination and dip; as, however, the maximum disturbance of the total magnetic force and a maximum of the magnetic dip appear to occur about 5^h P.M., this also may be an epoch of maximum frequency or intensity, though this can only be determined in higher latitudes. It should also be remarked, that, since the epoch of maximum disturbance varies with season, so, therefore, it is probable will that of frequency of the aurora; some traces of this may be deduced from the previous table. In the winter quarter, November-January, four-fifths of the times at which auroræ were seen were for the hours *before* 10^h P.M., whereas in the spring quarter there were only three-fifths seen *before* 10^h P.M.

Annual Variation of frequency of the Aurora Borealis.—The first line following contains the numbers of auroræ observed in each month during the six complete years 1843-8, and the second line gives the numbers of hours at which the auroræ were seen.

Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
15	16	26	14	6	0	0	7	13	27	23	11
50	62	65	43	8	0	0	10	32	44	58	38

The greatest number of auroræ were observed in March for the first six months, and in October for the last six months of the year: none were observed in June and July. When the six months of 1849 are included, the number for February is 26, and for March, 28. The law of visible frequency of the aurora is the same as that deduced already for magnetic disturbance; namely, maxima near the equinoxes, and minima near the solstices, the minimum at the summer solstice being the principal. As, however, the shortness of night during the summer months must diminish the number of visible auroræ, it is by no means certain from these numbers that a minimum occurs at the summer solstice; the fact of the minimum at the winter solstice is involved in no such difficulty. If we could assume that the auroræ had the same diurnal law of frequency at all seasons of the year, the existence of the summer minimum could be satisfactorily determined, by comparing the numbers of times which auroræ were seen at the five hours, 10^h P.M.—2^h A.M., during which (even in the months of August and May) there is little twilight to extinguish auroræ. The numbers are as follow, for these five hours in each month of the years 1843–8:—

Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
15	24	38	31	8	0	0	9	14	16	18	12

From these it is evident that the numbers in May and August are certainly less than for April and September; but it has been already mentioned as probable that the diurnal law of frequency varies with season, of which, indeed, a proof is to be found in the great excess of the numbers above for the spring months, compared with those for the autumn months, shewing the later epoch of the maximum frequency in the former. An examination, however, of the table for the disturbance of the magnetic declination (Table 18, Vol. xix., Part 2), will shew that, though the maximum disturbance occurs after midnight, in the months of May, June, and July; yet in August and the two following months it occurs about 10^h P.M., so that there can be no doubt of the less number for August than for September and October, if there should be a doubt in the case of May compared with April. The difference, however, even in the latter case is too great to be explained by any slight shift of the epoch of maximum

frequency in the two months. Upon the whole, it appears certain that a minimum of actual as well as of visible frequency occurs in summer; a result quite in accordance with that for the amount of magnetic disturbance, which accordance is sufficiently close to permit us to complete it, by assuming that the number of auroræ is a *principal* minimum in summer.

It has been stated in the volume for 1844, p. 401, that this result was long ago obtained by Mairan; this statement, made chiefly on the authority of Kæmtz and Hansteen, is not quite accurate. It is true that Mairan's numbers give a rough indication of the law, as will be seen below; but when it is remembered that his table includes all the observations (229) of which he could find a record for upwards 1000 years, it will be evident, that the conclusion that a greater number of auroræ occurred at both equinoxes than at the winter solstice would have been hasty; this conclusion, however, is *not* made by Mairan, and, though he has combined the numbers of auroræ in a great variety of ways, he has made no combination exhibiting this fact. It did not enter into the necessities of his theory (that auroræ are the product of the solar atmosphere) to shew that a greater number of auroræ happened in the northern hemisphere at the vernal equinox than at the winter solstice; he shews, indeed, that the number for one equinox is, and, in accordance with his theory, ought to be, greater than for the other. Some other philosopher has the merit of first pointing out this fact.

The following are the numbers of auroræ by Mairan (*Traité Physique et Historique de l'Aurore Boreale*, par M. de Mairan, 1733, p. 199); by Kæmtz (*Complete Course of Meteorology*, translation by Walker, p. 458); and by Hansteen (*Mem. de l'Acad. Roy. de Belgique*, t. xx., p. 117).

	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Sum.
Mairan, .	21	27	22	12	1	5	7	9	34	50	26	15	229
Kæmtz, .	229	307	440	312	184	65	87	217	405	497	285	225	3253
Hansteen, .	29	31	47	34	2	0	0	17	35	33	34	23	285
J. A. Broun,	22	26	28	16	6	0	0	7	16	29	23	11	184
Sum of last three, }	280	364	515	362	192	65	87	241	456	559	342	259	3722

Mairan's numbers are probably included by Kæmtz; a few of the auroræ, included in M. Hansteen's list, are identical with those in my own.

Variation of Frequency of the Aurora Borealis with the Moon's Age.—This investigation is evidently beset with considerable difficulty, since the moonlight existing nearly extinguishes the appearances of all the fainter class of auroræ, and it renders the faintest wholly invisible; the careful watch, however, which was kept for auroral appearances at Makerstoun, probably renders the table given in the Transactions better fitted for such a question than any previous series of observations.

It should be remarked, that the latitude of Makerstoun, or perhaps even a lower latitude, is better fitted for this investigation, than much higher latitudes; at least this is the case as long as only frequency of visibility can be considered. The French *Commission du Nord*, during their stay in Lapland, found auroræ existing, or probably existing, almost every night. In such places variation of frequency there is none, and variation of intensity alone remains for investigation. It is obvious, that till some better mode of measuring this intensity can be devised for these high latitudes, we are forced to perform this operation in a rude manner, by moving to lower latitudes, where the fainter auroræ become invisible, and where, therefore, frequency is a test of intensity beyond a certain limit.

Combining the numbers of auroræ observed at each day of the moon's age into six groups of 5 days (the first group, $4\frac{1}{2}$ days), we find the average number of auroræ for one day of the moon's age in each group as follows, from the $6\frac{1}{2}$ years' observations:—

Moon's age.	28 ^d —2 ^d	3 ^d —7 ^d	8 ^d —12 ^d	13 ^d —17 ^d	18 ^d —22 ^d	23 ^d —27 ^d
Number.	5·8	5·2	3·6	5·0	10·2	6·6

Did auroræ occur indifferently at all ages of the moon, we should expect to see the greatest number at conjunction, and the least number at opposition; this, however, is not the case, the greatest number was seen about two days before the end of the third quarter, and the least number about two days after the first quarter, or the visible maximum and minimum occurred at times *equidistant* from the epoch of opposition. The frequency of auroræ, therefore, is a function of the moon's age. In order to determine the actual law, we may consider the probable effect of moonlight in obliterating the auroral appearances; remarking, first, that 9^h P.M. is the epoch of maximum frequency for the aurora, and that upwards of five-sixths are seen before midnight. When the moon is about three days old, in the months from September to March, it begins to set sufficiently

late, and to have sufficient light to render the earlier of the faint auroræ invisible; about the end of the first quarter, it does not set till midnight, and thus shines throughout the period of the occurrence of five-sixths of the auroræ; afterwards it increases in brightness, and the maximum effect in extinguishing faint auroræ is evidently attained at opposition, when the moon begins to rise late enough to allow the earlier auroræ to be visible; towards the end of the third quarter, when the moon does not rise till midnight, it is also evident that the number of faint auroræ rendered invisible must be very small. From the beginning of the fourth quarter, therefore, till conjunction, the numbers *seen* will obey nearly the true law of frequency; and as the visible maximum occurred before the end of the third quarter, the true maximum must have occurred even nearer to opposition. On the whole, it appears very certain, that the hypothesis of an actual maximum of frequency at opposition, and minimum at conjunction, is satisfied by the previous numbers of auroræ, seen under the conditions of the varying duration of moonlight for the hours of maximum frequency. This hypothesis is in unison with the law of magnetic disturbance, which is a maximum at opposition, and a minimum at conjunction.

Note on the Theory of the Aurora.

Although temptations to frame hypotheses have been avoided hitherto, I cannot refrain from repeating here the opinion, that the phenomena of the aurora borealis are chiefly optical.

After watching the various phases of the aurora for some years, the hypothesis of self-luminous beams and arches appeared to me unsatisfactory; and the strongest argument in its favour, that obtained from the computed height of the auroral arches, seemed of a very doubtful character. I was quite prepared, therefore, to adopt the idea, first I believe proposed by M. Morlet to the French Academy, in May 1847, that the auroral arch is an optical phenomenon of position. M. Morlet has pointed out that the arch appears generally as a segment of a circle; whereas, in these latitudes, it ought invariably to appear as the segment of an ellipse, if the hypothesis be true of a real luminous ring, with its centre on the continuation of the magnetic pole. He has also, among many other very obvious objections to that hypothesis, shewn that the summit of the arch is generally in the magnetic meridian of the place, the plane of which

rarely passes through the magnetic pole, and seldom passes through the same point, for three different places. I have, however, felt even more persuaded that the aurora is, partly at least, an optical phenomenon, from a consideration of that phase of the aurora constituting the corona borealis, a persuasion that I stated in the Literary Gazette of the time, in giving an account of the beautiful corona of October 24, 1847.

Mairan, and, more lately, Dalton, have explained this phase of the aurora by a hypothesis of polar beams, long fiery rods of solar atmosphere, according to the one, of red-hot ferruginous particles, according to the other, seen in perspective, as they lie in the direction of the magnetic force. A little acquaintance with the phenomenon—the rushing and tilting of the beams against each other, one beam occasionally rising from the horizon, passing through the centre of the crown and beyond it—would shew the improbability of this hypothesis. I am persuaded, that the phenomena of the corona borealis is produced in a narrow horizontal stratum of the earth's atmosphere. Thanks to the discoveries of Dr Faraday, we do not require a ferruginous sea, in order to have polarized particles; the watery crystals that inhabit the upper regions of the atmosphere can themselves assume a polar state, determined by the passage of electric currents; and we have only to complete this fact by a hypothesis of luminous electric discharges seen refracted by these crystals, the position of visibility of the refracted rays depending on the angles of the crystals, and the deflections from the direction of the magnetic force which they suffer, by the electric currents. Such a hypothesis, which occurs at once when an optical phenomenon has to be accounted for, would explain these remarkable auroral clouds, so often seen in connection with the aurora itself; it would also serve to explain the appearance of the arch at certain altitudes, lower for lower altitudes, determined by the position of the source of light, direction of the magnetic force at the place, and the effect of the electric current in deflecting the crystals. The crystals successively deflected by electric currents would also exhibit the rushing pencils or beams.

It need scarcely be remarked, that differently formed crystals might give rise to different phases of the phenomenon; while reflection might be combined with refraction in certain cases, especially in the case of arches seen south of the anti-dip. Such a hypothesis evidently assumes a source of light, independent of these optical re-

sultants, and the pulsations seen in many auroræ may be real luminosities.

It is hazardous, in the present ill-arranged state of auroral observation, to offer so rude a sketch of a new hypothesis, although we may suffer a considerable defeat in very good company.

Since the previous note was written, I find that M. Morlet has published a theory of the auroral arch (*Ann. de Ch.*, t. xxvii., 3me Série). The ideas above were stated by me two years ago, to different persons.

The following Donations to the Library were announced :

Transactions of the Royal Scottish Society of Arts. Vol. III., Part 4. 8vo.—*By the Society.*

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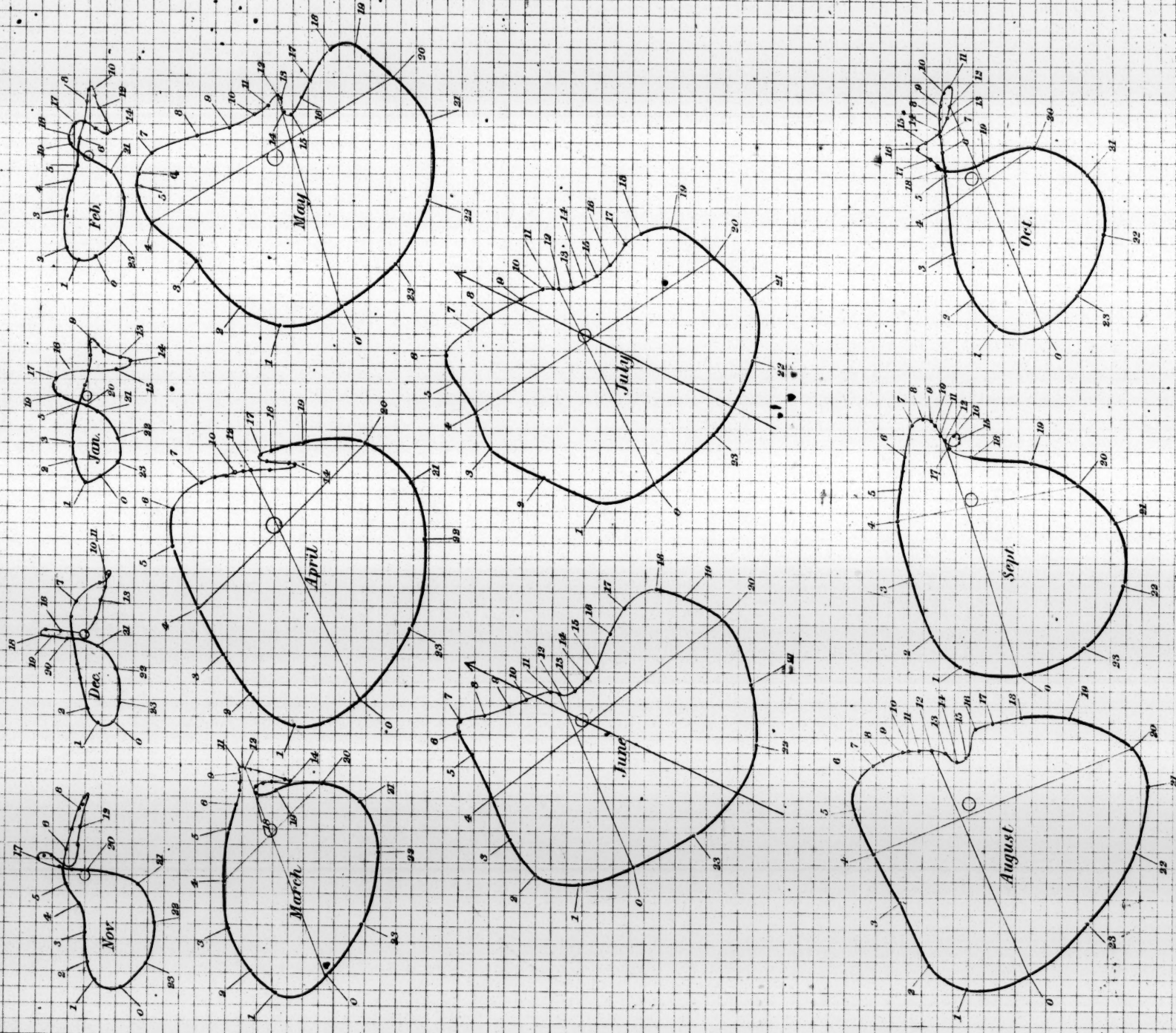
Bulletin de la Société de Géographie. 3me Série. Tom. 12me. 1849. 8vo.—*By the Society.*

ERRATUM,

Vol. II., No. 33, page 205, line 12 from bottom,

For "These data are (1.)," &c., read "These data are (1.), The known expansion of water in freezing; (2.), The known quantity of heat which becomes latent in the melting of ice; and (3.), The quantity of work given out," &c.





Scale 1 division = 0.2 minutes

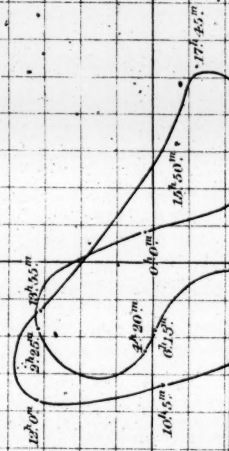
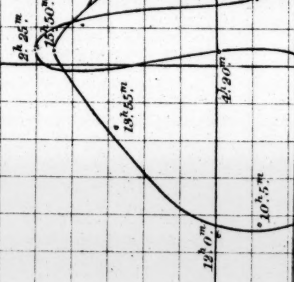
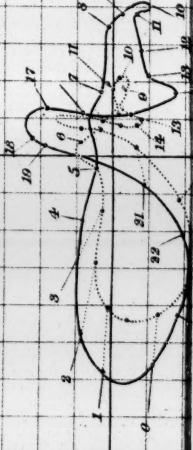
Motions with reference to the Moon's hour angles

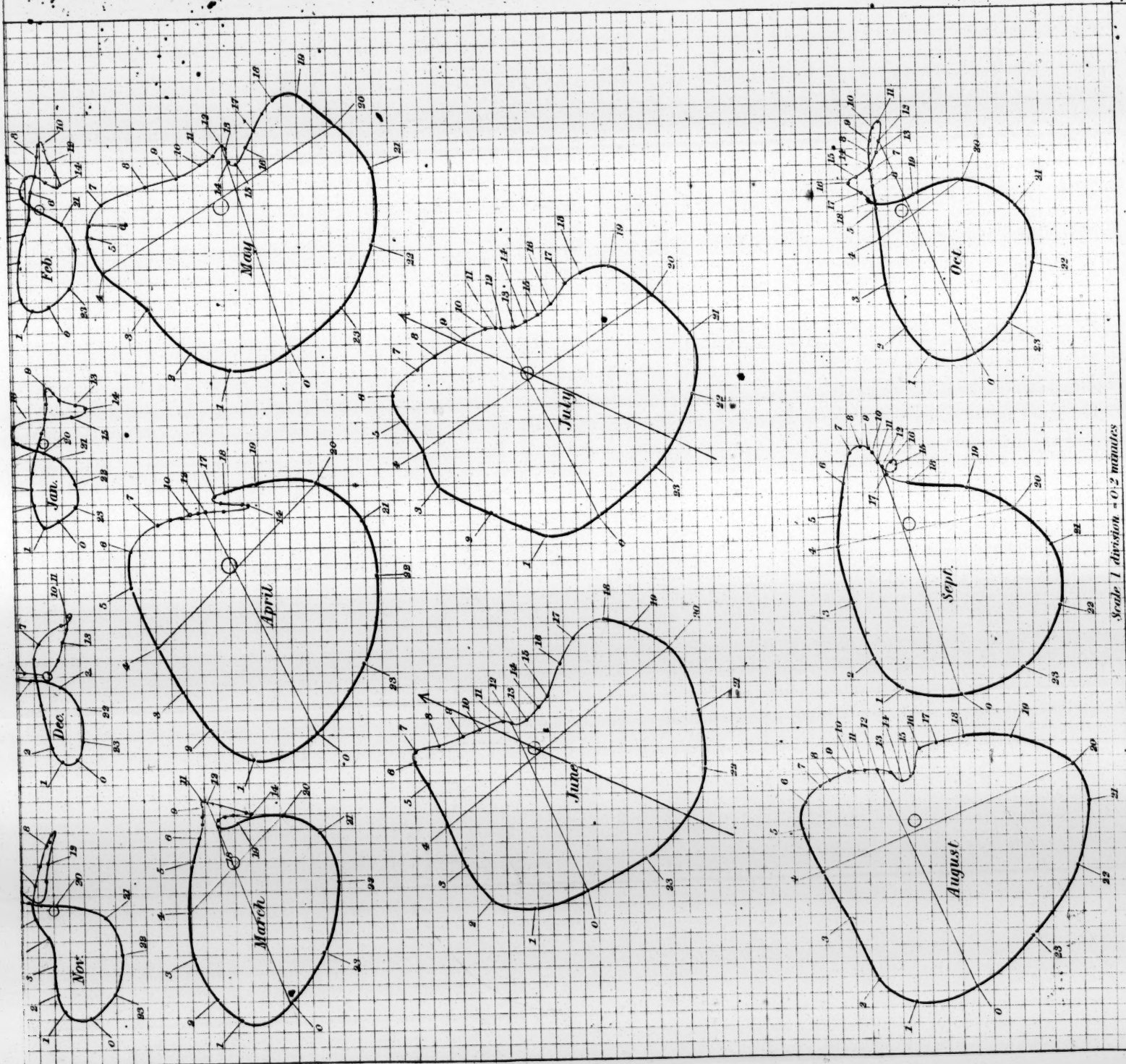
See Plate VIII.

Winter Lunations of 1845.

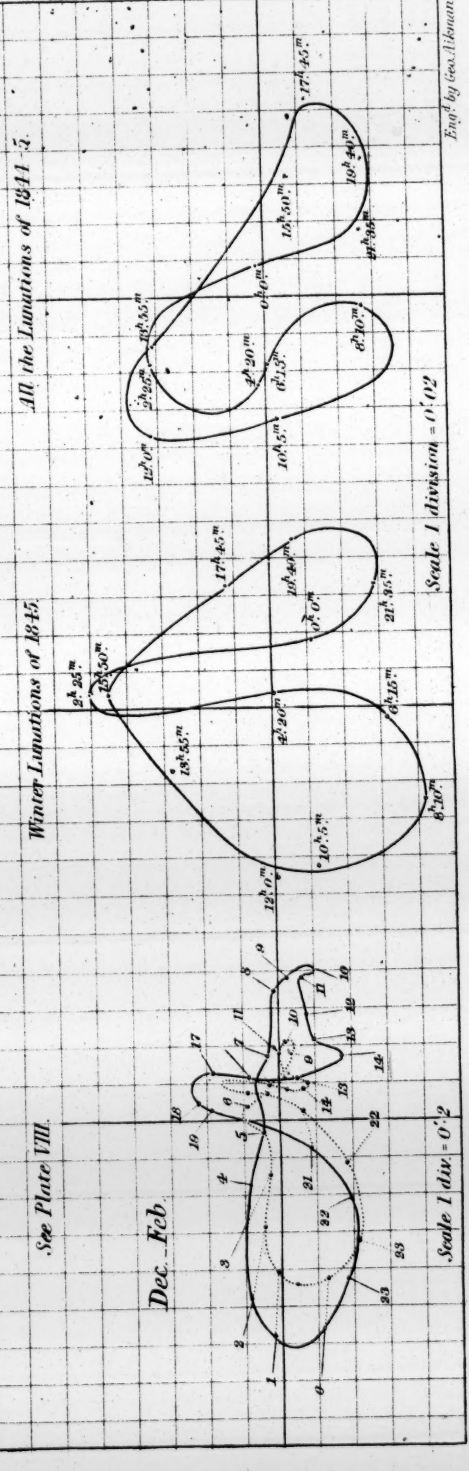
All the Lunations of 1844-5.

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